regions. Suppose, *e.g.* that in some region the rainfall in a long series of years varied, not as in the cases above considered, but in a certain regular correspondence with the sunspot curve; and in another region (perhaps further south) in opposite correspondence; also that these variations were traced to the shifting of a depression path. The opposite correspondence would obviously not be a good reason for denying sunspot influence, but rather corroborative evidence of such influence. Again, it will be admitted, I think, as conceivable that we might find certain great anticyclonic systems to vary in position or extent with the sunspot variations. Suppose, then, an anticyclone which lay over a region (*a*) at the time of minimum sunspots, were moved in a given direction, say northwards, so that it came to cover a region (*b*) at the maximum of sunspots and that it returned to *a* by the next minimum. In that case a place, *e.g.*, in the south part of region *a*, would have high barometer at minimum sunspots, while a place in the north part of region *b* would have low barometer. And at the maximum of sunspots, on the other hand, the two places would again have opposite conditions of pressure (to each other and to the first). These are some out of many aspects of the matter which seem to me to render doubtful the affirmation that if the solar cycle influences weather, it cannot produce an opposite succession of effects in different (even neighbouring) regions.

To revert, for a moment, to the shifting of depression-paths, might it not, in some cases, account for certain changes observed in the relative proportion of different wind directions? Suppose *e.g.* that, by the shifting of a path a little southwards, a place which has been for some years in its southern border comes to lie in the northern border, might it not thus come to have more easterly wind and less westerly? A. B. M.

The Non-Inheritance of Acquired Characters.

DR. WALLACE, in a letter which appeared in NATURE on July 20, asks for the opinion of naturalists as to the interpretation of certain facts bearing upon the question of the "Non-inheritance of Acquired Characters," and as I have given much thought to the subject I venture to offer my opinion.

In two papers published in *Natural Science*, vol. i. (1892), I set forth at some length a theory of heredity which has hitherto, so far as I am aware, met with no public criticism, and which I believe sets the question at rest, not by establishing the views of either of the rival schools associated with the names of Weismann and Lamarck respectively, but by showing that another interpretation is possible, and one which while fundamentally opposed to both of these makes it possible that there may be some truth hidden in the almost meaningless statements of the Weismannians and of the Lamarckians alike.

Till "heredity" is defined, and till we know exactly what we mean by "inheritance of characters" (be they "acquired" or "blastogenic"), it is useless to argue as to whether characters are "inherited" or not.

Is the word "heredity" an abstract noun, the name of a quality, a sort of magnified "family-likeness," or is it not? Those who write of heredity are too prone to speak of "heredity" as if it were a force or combination of forces producing an effect; as an "inherent tendency," to resemble parents or other ancestors which it is perhaps not unfair to compare to the "inherent tendency" of a watch to tell the time or of a weathercock to point to the south-west. There are those who even speak of it as being "latent" for a time and then, owing to some unknown cause, "springing into activity" anew and giving rise to what we call "atavism." Even "atavism" is not infrequently spoken of, as if it were of the nature of a force or combination of forces, comparable to a "latent tendency," which after lying "dormant" or "latent" for a time in a weathercock, suddenly springs into new activity and causes it to point as of old to the south-west.

It appears to me that if we once grasp the idea that "heredity" is the name of a quality, a particular kind of "likeness" or "similarity," and nothing else, we shall be saved from much useless discussion of propositions which are intrinsically almost, if not quite, meaningless.

Artemia salina is the collective name given to a large number of individuals which have certain characters in common. It would hardly seem to be necessary to suggest the probability that this possession of many characters in common is due to the action of Natural Selection; that each new individual possesses the characters in question solely by virtue of the fact that Natural Selection has led to the production of individuals possessing the power to produce, under given constant conditions, eggs, which by virtue of their constitution will develop under given conditions into adults possessing the characters which natural selection has *under those conditions* rendered nearly constant.

It has been found that this same constitution does not necessarily lead to the same series of developmental changes *under other conditions*, and that in strong brine the eggs develop into animals which, though capable of living and multiplying under those conditions, differ in form from the ancestral *A. salina*. This new form has no more right to rank as a species than has a "worker" bee whose adult form differs from that of its parent merely on account of certain conditions to which it is exposed during development.

It appears to me to be absurd to ask whether the "acquired characters" of the so-called *Artemia Milhausenii* are inheritable or not. Experiment has shown that the constitution of the species *A. salina* has so little changed that it still has the power to produce eggs which under one set of conditions develop into *A. salina* and under another set of conditions into *A. Milhausenii*. The average constitution of the species has not varied: it still produces ova which will develop into either *A. salina* or *A. Milhausenii*, according to the conditions to which it is exposed. If we look upon the species as a whole, it is not too much to say that it exhibits *no acquired characters*. If bred in strong brine the individuals of many generations are alike, having been moulded by like influences, intrinsic as well as extrinsic. If the extrinsic influences change, new individuals differ from the old ones, simply because the constitution of the individuals as well as that of the species is such that under the new conditions the developmental changes occurring differ from those which would have occurred under the old conditions.

Whether this is true of all species and under all conditions consistent with life and multiplication, or is not true of some, is a matter for experiment, and can never be decided by argument. The experiment has been made by nature, and also by man in the case of *Amblystoma*, and with a result in exact conformity with the result in the case of *Artemia*.

The experiment has also been made with *white mice* in Freiburg, and it has been conclusively shown that under constant conditions the characters of successive generations are constant. One element of the environment in one series of cases was Prof. Weismann armed with tools for amputation of the the tails of the young mice, *plus* a determination to amputate those tails. So long as this remained a constant factor in the environment, so long and no longer did the taillessness of the adult mice remain a constant character of the species.

The Texan species of *Saturnia*, so long as the exclusive supply of *Juglans regia* is a constant factor in the environment, may or may not have a constant group of characters. That is a matter for experiment; but innumerable experiments, called collectively "domestication," have shown that whatever effect changes of certain details of the environment—such as food—may have, the suspension of natural selection will in the long run lead to inconstancy of all those characters which are relieved from its restraining influence.

If anything has ever been rendered certain in biology by prolonged experiment and observation, it is the fact that specific characters are maintained constant *by selection and by that alone*. Long continued selection—natural or artificial—may produce a seeming constancy of characters (*which we call "heredity"* !), but in the long run this constancy will vanish when the particular selection which has induced it has been suspended for a sufficiently long period.

The discusssion upon the "Inheritance of Acquired Characters," though it has led to many valuable results, has been throughout little more than a quibble, for in the whole discussion, so far as I am aware, the meaning of the word "inheritance" has never been defined. Most of the disputants appear to use the term as the name for the action of a force or combination of forces, which some have called "heredity"—a force or influence—either simple or complex—of which it is perfectly safe to deny the existence. There is no such *thing* as heredity—heredity is only a quality, a likeness or similarity, and nothing more. That likeness of characters is simply and solely due to the likeness of the influences which have produced the like characters, and pre-eminent amongst those influences is natural selection, though *every* factor of the environment has also had its part to play. So long as those like influences—

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extrinsic and intrinsic—remain like, so long and no longer do their effects remain constant. What effects a change in those influences may produce experiment and observation, and these alone can determine.

For thousands of generations certain characters, such as blindness and winglessness, and enormous size of head and of jaws, and complete absence of all power of multiplication either direct or indirect, either sexual or asexual, have remained constant in "soldier" termites. No soldier termite has any marked resemblance to any one of its ancestors. Yet each is like every other "soldier," and that likeness is "heredity." Its characters have not been "transmitted," for no ancestor ever possessed them. Like conditions in successive generations lead to like results, one of which is the production of a fairly constant proportion of neuter "soldiers" and "workers" devoid of any power of reproduction. Those "like conditions" include a constant, or nearly constant, structure of the males and females, albeit the polymorphism extends to these also. The recurrence of those like conditions has been determined by natural selection, and is still maintained by natural selection. To apply the term "transmission" in such a case would be not more absurd than in any other case, supposing the word to be used in its literal sense, for, though the soldier in every case derives its characters from a line of ancestors extending back to remote periods, not one ancestor in which line has possessed those characters; yet their geographical distribution at the present time shows that the characters of "soldiers" are of remote antiquity. This constancy of characters in many generations appears to be identical with the phenomenon called "inheritance."

Heredity then is a likeness of effects due to likeness in the causes producing them. The likeness of causes has been produced and maintained by natural selection acting under fairly constant conditions. "Inheritance" is a name given to the operation of an influence which has no existence in nature. The sooner we cease to use the word altogether the better it will be for our science.

"Inheritance of acquired characters" is a mere chain of words correlated with a chain of loose ideas, but not correlated with any natural objective phenomenon. To assert it as a fact is as futile as to deny it. C. Herbert Hurst.

Owens College, Manchester, July 26.

Echinocyamus Pusillus.

May I direct your attention to a rather serious error contained in the review of Théel's paper on *Echinocyamus pusillus*, which appeared in your number for August 3?

Your reviewer states that "not the least brilliant and far-reaching" of the advances in our knowledge of Echinoderm morphology, made in the year 1891, is the discovery by Brooks and Field of the primary bilateral symmetry "of the water-vascular system" of Asterias.

Had such a discovery been really made, it would no doubt have justified the epithet applied to it by your reviewer; but, in the first place it was not made, and in the second Metschnikoff long ago pointed out such a primary bilateral symmetry in the embryos of *Amphiura squamata*.

Field's paper, containing the results of his own and Brooks's work, appeared in the *Quarterly Journal of Microscopical Science* for 1892. In it he gives an account of the development of the larva of Asterias; but in the oldest stage which he describes *there is not as yet a trace of the water-vascular system*. He describes, it is true, in larvæ of a certain size a right as well as a left madreporic pore; but as all "echinologists," it is to be hoped, know by this time, the pore is primarily related to the cœlom, and only secondarily enters into connection with the water-vascular system. Further, Field distinctly states that cases of a double pore had been observed previously by continental zoologists, but regarded by them as pathological; and the chief point in Field's paper is the very probable theory put forward by him that such cases constitute a distinct stage in the ontogeny of the animal. E. W. MacBride.

Zoological Laboratory, New Museums, Cambridge,
August 5.

On referring to my draft notes, which I happen to have kept, I find in place of the words "water-vascular system," quoted by your correspondent, "pore canal system"; and I do not deny that I should have done better had I transcribed them unchanged. I fail at the same time to see that the "error" complained of is, in its place, as serious as my critic would imply. No one who attempts to do his duty by the colossus of biological literature would fail to be familiar either with the work of the authors whom he cites, or with his own recent beginning in a kindred direction. Admitting the claims of one and all, the work of Field appeared to me to put the probability at stake upon a much firmer basis than that of his predecessors, and it was for that reason that I emphasised it. If I err not, a journalistic notice is not a thing to be hampered with names and details, especially when written with the dual object of directing attention to a really admirable monograph, and of endeavouring to promote an amicable spirit of brotherhood among workers in science, such as we to-day very much need. The Writer of the Notice.

The Supposed Suicide of Rattlesnakes.

The letter of Mr. Edward S. Holden on this subject is extremely interesting. It appears that he, like other individuals who have imagined that they have witnessed the suicide of scorpions, has fallen into the error (so common in the interpretation of biological as distinguished from abiological phenomena) of stating his inferences and beliefs as though they were observations. The "instance which occurred before my eyes" (to quote his words, which remind one of the old herbalist, Gerard) was simply that of a snake biting itself when imprisoned in a jar of water. That the blow was "deliberate," "intentional," and of "suicidal purpose" is pure speculation—and nothing occurred before Mr. Holden's eyes to warrant his entertaining such a notion. Had Mr. Holden been aware that the poison of the rattlesnake has little or no effect upon another rattlesnake, nor upon the individual from which the poison is furnished, he would probably have been less ready to conclude that the bite was one of suicidal purpose. He would then perhaps have inquired as to the depth to which the bite penetrated into the tissues of the snake, and how far such a superficial bite as a snake can inflict upon a part of its own body is likely (in the absence of any poisonous action) to be seriously injurious to the snake.

In this case, as in that of the scorpion confined in a fiery circle (experimentally studied both by myself and by Prof. Bourne, of Madras, and reported on in the Proceedings of the Linnean Society and the Royal Society) the spasmodic struggles of an animal artificially confined and tortured, have been, as we clearly demonstrated, mistaken for efforts at self-destruction. The biting of its own body by the snake may be justly compared with the "biting the dust" attributed to men who die in hand-to-hand struggle, or to the biting of their own hand or arm by unhealthy children when suffering from a paroxysm of anger. E. Ray Lankester.

Oxford, August 11.

Imitation or "Instinct" by a Male Thrush?

On the evening of July 19 a young thrush was caught in my conservatory and placed in a large outside aviary. The following morning I observed the parent birds feeding the young one through the bars with worms. In the same aviary there had been for more than ten years a male thrush which had been captured when quite young and had never been mated or troubled with family cares. On observing the parents of the young bird feeding their offspring he at once followed their example. On putting some bread and milk into the aviary he flew down, took up a piece and tried to induce the young bird to open its beak. At first the young thrush appeared to be afraid of accepting food from the foster-father, but after some persuasion it allowed itself to be fed with bread and milk, hemp seed, and other food. The parent birds were watching from the outside, and during the whole time occupied by the old male in feeding their progeny were also trying to introduce food through the bars. The day after (July 21) the parent birds did not make any further attempt to introduce food, but contented themselves by watching their young one from a tree close at hand. If any of the house-cats approached the aviary the parents would at once give the alarm. In the course of another day or two they abandoned the young one entirely to the care of the old foster-father, who has proved quite worthy of his trust, as the young bird is now able to feed himself and is in a very thriving condition. The old male still insists, how-

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